

have been published during the four past years. If, as we suppose, about twenty more parts are required to finish the work, it is manifest that unless the present rate of progress be expedited it will be twenty years before we are able to send our new "History of British Birds" to the binders. The edition was commenced, we believe, in 1871. Now thirty years seems rather long for the execution of a new edition of any work, even with all the improvements which, as we have shown above, the present editor has doubtless bestowed upon it. We would fain ask therefore whether the author and publisher cannot manage to move on a little faster. If this cannot be done it appears to us that the first portion of the work will be almost out of date before the last part is published, and that the subscribers will have good reason to complain.

OUR BOOK SHELF

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Elfter Band, drittes und viertes Heft. With twenty-four plates. (Leipzig: W. Engelmann, 1877 and 1878.)

DR. JAKOB ERIKSSON describes in a lengthened paper the protomeristem of the roots of Dicotyledons, and directs attention to the four great types of structure observable in these roots. In the first type the apex consists of three separate zones of meristem: the plerome, periblem, and dermocalyptrogen. In the second type only two zones are present: the plerome and a common zone for primary cortex, epidermis, and root-cap. In the third type there is a common meristem zone from which all the others develop; while in the fourth there are two zones, the periblem and the plerome. Two additional types are met with in Monocotyledons: (1) in which there are four zones of meristem: calyptrogen, dermatogen, periblem, and plerome; and (2) in which there are three zones: the calyptrogen, the plerome, and a common zone for cortex and epidermis.

The germination of Equisetum and Schizæacæ forms the subject of two papers, one by Sadebeck and the other by Bauke, whose work was arrested by premature death. Woronin contributes a paper on the *Plasmodiophora Brassicae*, the remarkable Myxomycete which seems to be the cause of the so-called Hernia of the cabbage plant, which has recently attracted so much attention.

The remaining papers are by Reinke, on *Monostroma bullosum* and *Tetraspora lubrica*. Wydler discusses at great length the morphology of certain forms of inflorescence, chiefly dichotomous; and lastly there is a paper by Pitra on the pressure in stems during the appearance of bleeding in plants. The contents of the parts are, as will be seen, very varied and deal with many different departments of botany, and will be found to sustain the reputation of the "Jahrbücher" so long associated with the name of Pringsheim.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Unconscious Memory—Mr. Samuel Butler

WILL you kindly allow me a portion of your valuable space in order that I may demonstrate the completely groundless character of a series of insinuations which Mr. Samuel Butler

has made not only against myself, but also against Mr. Charles Darwin, in the work which he has recently published, entitled "Unconscious Memory" (Op. 5).

1. Mr. Butler insinuates that Mr. Darwin caused my essay on Dr. Erasmus Darwin to be translated simply in order to throw discredit on his work, "Evolution, Old and New" (Op. 4), which was published in May, 1879. Upon this point I have to observe that Mr. Darwin informed me of his desire to have my essay published in English more than two months before the appearance of Mr. Butler's book; that the translation did not appear earlier is due to the fact that I asked for a delay in order that I might be able to revise it.

2. The assumption of Mr. Butler that Mr. Darwin had urged me to insert an underhand attack upon him (Mr. Butler) in my sketch, is not only absolutely unfounded, but, on the contrary, I have to state that Mr. Darwin specially solicited me to take no notice whatever of Mr. Butler's book, which had in the meantime appeared. Since however I thought it desirable to point out that Dr. Erasmus Darwin's views concerning the evolution of animated Nature still satisfy certain thinkers, even in our own day (a fact which must add greatly to Dr. Darwin's reputation), I have made some remarks upon the subject in a concluding paragraph, without however naming Mr. Butler. And I may here emphatically assert, that although Mr. Darwin recommended me to omit one or two passages from my work, he neither made nor suggested additions of any kind.

3. Mr. Butler's assertion that the revision of my translation was made "by the light" of his book is only in so far justifiable that I looked over the latter before sending off my work, and that my attention was thereby called to a remark of Buffon's. From Mr. Butler's book I have neither taken nor was I able to take the slightest information that was new to me concerning Dr. Erasmus Darwin's scientific work and views, since in it practically only one portion of the "Zoonomia" is discussed at any length, and this portion I had already quoted and analysed, while Mr. Butler only refers to one comparatively unimportant part of the "Botanic Garden," and absolutely ignores the "Phytologia" and the "Temple of Nature." So that no single line of Mr. Butler's far from profound work was of the slightest use to me.

Mr. Butler's contention that I have quoted from his book a remark from Coleridge is entirely without foundation. I have been acquainted with this remark for years, and from the source quoted. It is also quoted in Zoëckler's work (vol. ii. p. 256), mentioned by me on p. 151, which appeared prior to Mr. Butler's book (Op. 4). The whole of my indebtedness to Mr. Butler reduces itself therefore to a single quotation from Buffon.

4. Finally, as concerns the main accusation that no mention is made in the preface of the fact that my essay had been revised previously to publication, it is clear, as even a child could not fail to see, that this is not due to design, but is simply the result of an oversight. It would be simply absurd for a writer intentionally to attack a publication which appeared subsequently to the date indicated on his title-page; and the so-called falsification, so far from injuring Mr. Butler, could only be most agreeable to him, because it might induce the careless reader to fancy that no reference whatever was intended to Mr. Butler in the closing sentence. Should however such a reference be clearly intended—and to every reader posted up in the subject this could not be doubtful—every man of common sense would recognise this terrible falsehood to be a simple oversight.

Be'lin, January 12

ERNST KRAUSE

Hot Ice

I VENTURE, in referring to Dr. Lodge's letter of this week, to put before your readers the meaning of the remarks made on Dr. Carnelley's experiment at the Chemical Society by Prof. Ayrton, who is now away from England. I understood him to say that as Dr. Carnelley's hot ice is obviously in a condition which cannot be represented within the as yet known fundamental water surfaces, it is necessary to produce these surfaces beyond the places at which, hitherto, abrupt changes have been supposed to take place in them. He took as an instance the ice-water surface which has hitherto been assumed to stop at Prof. James Thomson's "triple point," and showed that although Sir Wm. Thomson's experiments have proved that it is nearly plain for the stable state of water and ice, yet in the imaginary district beyond the triple point a change of latent heat might give such a change of curvature as to bring this surface into the hot-ice region.

With Prof. Ayrton I have done for water what Prof. James Thomson did for carbonic acid; we constructed in stiff paper a surface or surfaces which represent the relations of p , v and t for a given quantity of water-stuff. Three parts of the whole are cylindrical surfaces and divide space into three regions; in one of them the substance is in the form of ice, in another in the form of water, in another in the form of vapour; and they meet in Thomson's triple point. Any one looking at this model must feel that Prof. Ayrton was right in looking for the hot-ice state in a region bounded by imaginary productions of the *all-ice*, the *all-water*, the *all-vapour*, and the above-mentioned three cylindrical surfaces beyond their lines of intersection. This is what Prof. James Thomson did to indicate the state of water before boiling by bumping begins. He assumed that the *all-water* surface changed into the *all-vapour* surface gradually, and not through a purely cylindrical *water-vapour* surface, and this is really what Dr. Lodge himself does for hot ice. That is, he imagines the *all-ice* surface to change into the *all-vapour* surface gradually, and not by sudden changes through a purely cylindrical *ice-vapour* surface. According to Mr. Ayrton the imaginary production is even of a more complicated kind than Dr. Lodge supposes, as the ice probably changes into unstable water before it changes into steam. There can be no doubt that such imaginary productions find their place in the fundamental equation of water, but I cannot agree with Dr. Lodge in thinking that we have at present an explanation of such unstable conditions. If his explanation were satisfactory we ought to be able in the same way to explain the unstable position which precedes boiling by bumping, and this we cannot do. Where the explanation seems to me to fail is in the assumption that the hot vapour filling a cavity, being of lower temperature than the surface of the cavity, is always at a pressure less than that of saturation, in spite of the evaporation going on. Now when we consider how large the surface of a minute cavity is as compared with its volume, the very great increase in bulk when the solid is changed into vapour and the lowering of temperature which the surface must undergo on account of latent heat, we see that the condition which Dr. Lodge assumes to be maintained during the whole experiment would be instantaneously destroyed in a very minute cavity. In explaining hot ice I am afraid that neither Prof. Ayrton nor Dr. Lodge has given us more than Prof. James Thomson has given in explaining "boiling by bumping." The cause of the phenomenon is a molecular one probably, and must be left to the guesses of molecular physicists.

JOHN PERRY

14, Talgarth Road, West Kensington

Mr. Bottomley's Experiments with Vacuum Tubes and the Aurora

MR. BOTTOMLEY'S extremely interesting experiments briefly described in NATURE, vol. xxiii. pp. 218 and 243, appear to have a very important bearing on the question of atmospheric electricity; for if such high vacua are good conductors of electricity we have reason for thinking that the electrical conditions of our globe will be very different from what we have been accustomed to regard them. The layers of denser air surrounding the conducting matter of the globe will act like the glass of Mr. Bottomley's tubes in maintaining by a Leyden-jar-like action any difference of potential that there may be between their inner and their outer surface. Again, in the piercing of the glass tube by a minute spark, we have the analogue of the lightning flash between the clouds and the earth; the insulating layer in each case giving way, when, owing to an excessive increase in the surface density of the charge at any point, the dielectric stress exceeds the limits of the dielectric strength of the medium. The internal luminous effects observed by Mr. Bottomley as the result of change in the distribution of the external charge of electricity will be the physical analogues of the *aurora*, with this difference, that they take place in the ultra-gaseous interior, whereas in the case of our globe the luminous phenomena take place in the ultra-gaseous (*i.e.* highly rarefied) exterior regions of the atmosphere. It would be interesting to learn whether such discharges present any other analogies with auroral phenomena. I should be particularly interested in learning whether the conditions under which such luminous effects are obtained give any support to the theory which I think to be the only consistent one, that the aurora is due *not* to electrical discharges from regions of less atmospheric density to regions of a greater density (or *vice versa*), but to electrical discharges in a region of pretty uniform (and small) density, and in which

region differences of electric potential exist. According to this view the auroral streaks which appear to be radial should in reality lie approximately parallel to the earth's surface, and not stand (as most persons imagine) normal to it. A series of horizontal parallel lines drawn across the sky in a direction approximately north and south would necessarily appear to an observer on the earth's surface foreshortened into a set of lines diverging in fan-like forms at either the north point or the south point of the horizon. Their divergence would therefore be apparent only, like the "beams" diverging from the sun at sunset on a cloudy day, or like the beams of the *rayons du crépuscule*, or like the "radial streaks" which I have pointed out as frequently accompanying rainbows.

SILVANUS P. THOMPSON

University College, Bristol, January 22

P.S.—The behaviour of a hollow sealed glass tube containing a conducting substance in its interior was noticed just one hundred years ago by Cavallo, who sealed up a glass tube in which mercury was at its boiling-point, thus obtaining a fairly perfect vacuum.—S. P. T.

The Geological Age of the North Highlands of Scotland

FROM the abstract of *Proceedings* of the Geological Society (January 5) I learn with surprise that Sir R. Murchison's interpretation of the succession of the beds over the region north of the Caledonian Canal is disputed, and that the relations of the fossiliferous limestone of Durness to the quartzites "are" (according to Dr. Callaway) "by no means satisfactorily established, and that their conformity is rendered dubious by a marked discordance of strike"; in fact that the limestone lies in a synclinal basin amongst the quartzites, so that if the limestone be of Lower Silurian ("Arenig") age the quartzites and schists must be older; this I presume to be the inference Dr. Callaway intends to draw, as he says there "is no proof of the Lower Silurian age of the quartzite and newer series of flaggy gneiss and schist" constituting the interior mountainous district.

Having had an opportunity last spring of visiting the district lying between Lochs Broom and Inchard under the guidance of Prof. Geikie and in company with my colleague of the Irish Survey, Mr. Symes, I take the opportunity offered by Dr. Callaway's paper of expressing my entire concurrence in the interpretation of the structure of the country given by my late chief, whose elaborate and graphic descriptions in the pages of the *Quarterly Journal* of the Geological Society (vols. xv. and xvii.) will, I feel sure, never be invalidated.

After seeing the clear infra-position of the limestone to the upper quartzite and schists first in the section at the Bridge of Ault-Corry near Ullapool, then in the cliffs near Ullapool, next at Inchadamff and the head of Loch Assynt, then again in the Forest of Arkle and the hills bordering Loch Stack, where the limestone band is clearly interbedded between the lower and upper quartzites, and this latter as clearly passes under the schists of the interior, it required no further evidence to prove that all these beds belong to one conformable formation; and that the geological age of the whole group is determined by the fossils discovered by Mr. Charles Peach in the limestone of Durness or Assynt, and named by the late Mr. Salter. The geological sequence is so clear throughout that region, and so entirely bears out the description given by Murchison and Geikie, that "he who runneth may read"; and I have no hesitation in saying that the evidence that the Millstone Grit overlies the Carboniferous Limestone, and that the New Red Marl overlies the New Red Sandstone is not more clear than that the upper quartzites and schists overlie the Assynt limestone.

I wish to point out in conclusion that the trough-shaped arrangement of the Durness limestone and its faulted position, described by Dr. Callaway, has already been described by Murchison in the *Quarterly Journal*, vol. xv. Any one visiting the grand tract of country lying between Durness and Loch Maree need have no better guide than the papers I have referred to, and a good geological map. He will find that there is little, if anything, to add to the details and conclusions there given, and were it not that Dr. Callaway's objections seem to find support with some geologists of more experience than himself, it would not have been necessary to enter a caveat against them.

As regards the question whether in any part of the Highlands of Scotland except along the western coast the Laurentian (or "pre-Cambrian") rocks reappear, as has been stated or suggested, I do not wish to offer an opinion. As regards the region